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THE INTERIOR OF THE EARTH VIEWED IN ITS RELATION TO EARTHQUAKE CAUSES

THE VIEWPOINT OF SEISMOLOGY*

By ERNEST A. HODGSON

IN accepting a commission to present a paper on a prescribed subject, a speaker tacitly reserves the right to make his own interpretation of the title assigned. Let us examine the limitations which one might define when considering, from the viewpoint of seismology, the symposium subject: "The Interior of the Earth Viewed in Its Relation to Earthquake Causes".

The first limitation which seems justified is that the discussion should be confined to that part of the earth's interior where conditions of potentials exist which do or which may contribute to the cause of earthquakes. As most earthquakes originate at depths ranging from, say, a kilometer to about 700 km., one might at first thought decide to consider only the spherical shell within the earth which is bounded by these two horizons.

That there is ample scope for discussion even with this stricture may be taken for granted. It would be desirable, however, to extend the depth range to include, for secondary consideration, those horizons in which conditions exist which contribute to the data on which are based the accepted theories as to the cause of earthquakes.

*A contribution to the Symposium on the Interior of the Earth Views in its Relation to Earthquake Causes held on the occasion of the Centenary Celebration of Fordham University, New York City, September 15-17, 1941; and printed here with the approval of Professor J. J. Lynch, S.J., director of the seismic observatory at Fordham University.—*Editor.*

If this be granted, the upper limit must be raised to the actual surface of the earth and the lower set at its very centre. For the mountains and the ocean basins, the geosynclines and the tectonic structures of the outer crustal layers are certainly surface indications which contribute to our deductions as to the cause of shallow-focus earthquakes. And the discontinuities, especially the great discontinuity at the core, together with the distribution of elastic properties and density give similar evidence on which are based the accepted conclusions as to the cause, and even the very existence, of deep foci.

It may be assumed, then, that the primary concern in this presentation is that range of depths in which earthquakes originate but that it is proper to admit the discussion of so much of the rest of the earth's interior as may contribute to an understanding of the cause of earthquakes. Further, since the viewpoint is that of seismology, the paper is limited to an examination of those data which have been obtained directly from studies of earthquakes and of their instrumental records.

Dutton, in 1904, expressed what was the accepted view of seismologists of his day when he set the maximum depth of earthquake foci at twenty miles. When, in 1921, Walker deduced probable depths as great as 800 mi., his conclusion did not find general acceptance. It was supported, however, by a series of papers by Turner, of which the first appeared in 1922. Wadati's first paper on the distinctions indicating the existence of shallow and deep-focus earthquakes appeared in 1928.

Since then, the evidence has grown from the studies of many; so that to-day seismologists generally accept the conclusion that earthquake foci may appear at depths as great as 700 km. One of the earlier papers, which served to convince even the most reluctant, was the doctorate thesis of Father Stechschulte, published in 1932. A paper by Scrase, based on studies carried out about the same time but published a few months earlier, set forth in some detail the characteristics of deep-focus earthquakes as shown by seismograph records. The graphs, published by Father Brunner in 1935, brought the means for ready evaluation of focal depths from seismograms into the hands of station operators, who were thereby able to prove readily for themselves, through the evidence shown by their own current records, the inescapable conclusion that earthquakes do originate in some cases

at very considerable depths, or "finite depth" as Walker put it two decades since. Father Macelwane gave the name "plutonic earthquakes" to this newly accepted class of foci. Within the past few years, a series of papers by Gutenberg and Richter has brought together an overwhelming mass of evidence concerning them, so that to-day any hypothesis of earth structure in the zone of earthquake foci must take cognizance of these modern data.

Accepting for the moment the statement that earthquakes originate at foci which may be classified as "shallow" and "deep", attention may be directed to the theories advanced to explain the cause of earthquakes. We shall consider initially only shallow-focus shocks, the so-called "normal" earthquakes, and may profitably pass over the early philosophical theories as to their cause, which range from the supernatural to the pseudo-scientific. One of the first inescapable deductions from the data obtained through the world-wide network of seismograph stations set up through the efforts of Milne, toward the end of the nineteenth century, was that they could not be generally due to volcanic action. The natural division into volcanic and tectonic, or structure building earthquakes, followed. It is with the cause of tectonic earthquakes that we here concern ourselves.

As observed by Leet, there is ample evidence to show that whatever be the origin of the forces at work to change the structure of the earth "it may be accepted as axiomatic that the cause of earthquakes is intimately related to the universal mobility of the earth's surface". Some suggestions which have been offered as to the mechanisms by which these forces might be built up are: The shrinkage of the earth due to cooling, the variation with latitude and depth of the horizontal component of centrifugal force ("Polfluchtkraft"), the transfer of sediments and the removal of great ice sheets. Certainly the cause of shallow-focus earthquakes is closely related to orogenic or epeirogenic forces. Critical examination of the above mechanisms by many specialists shows that each seems insufficient, by itself, to account for the evidences found by geologists on the surface of the earth. Each implies a rigid crustal layer of considerable strength overlying one, more or less deep, of weak, but dense and viscous, material. This concept is supported by the measurements of gravity and geodesy. We confine our attention to the evidence from earthquakes and their records.

In 1906 a great earthquake displaced the San Andreas fault in California throughout a surface span of more than 200 miles. Some horizontal displacements of adjacent edges of this fault were more than twenty feet. After studying the evidence obtained, Reid proposed the "elastic-rebound" theory to account for the data. According to this theory the forces, in this case largely horizontal, had been built up by some one or more of the mechanisms mentioned or by others not yet suspected. These forces tending to cause the shift were opposed by friction, thus storing up in the distorted edges of the fault the energy which was released at the time of the earthquake. This explanation is generally accepted in the case of normal earthquakes.

That some earthquakes result from forces with a strong vertical component is evidenced by many field studies. The vertical displacement in the Hawke Bay earthquake in New Zealand in 1931 was, in places, as great as eight feet; while in the Alaskan earthquake of 1899 the sudden vertical uplift was nearly fifty feet. It may be granted from field studies of shallow-focus earthquakes alone that both horizontal and vertical movements occur with many examples of a combination of these, resulting in oblique shifts at the exposed fault scarp. In each of these, neglecting the unsettled question of the mechanism which caused the strain to develop, it may be taken as a fact that the earthquake was a manifestation of a release of strain as set forth in Reid's theory of elastic rebound.

Other papers in this symposium deal with the subject of isotasy. The only observation to be made here is that the depth of isostatic compensation is not likely to be a discontinuity as evidenced by the travel times from earthquakes. Daly states that the elastic properties and the density of a rock changing from crystalline to vitreous conditions at depth vary in about the same ratio, so that a sudden transition in seismic wave velocity is not to be expected; and, according to Gutenberg, it cannot be observed. We are not, in this symposium, concerned directly with wave velocities except in so far as they reveal conditions which have some bearing on the cause of earthquakes. The Mohorovicic discontinuity, at a depth of 30-40 km., is, according to general seismological opinion, not to be identified with the depth of isostatic compensation at a depth variously estimated at from 10-120 km. but certainly well within the asthenosphere. Not more than two

decades past it was generally supposed that earthquake foci could not occur in the asthenosphere.

The seismological evidence that foci do occur at such depths is overwhelmingly conclusive. Walker deduced the fact from computed angles of emergence, based on data furnished by Galitzin. The attention of Turner was first directed to it by the too-early arrival times at the antipodes of seismic waves from certain foci which he defined as "deep". Two phases due to longitudinal waves reflected at the surface should occur if the focus is appreciably deep, as was pointed out by Walker as early as 1921. These two phases have been identified in many studies of records from plutonic earthquakes. There should be a marked reversal in curvature in the time-distance curves for deep foci. A number of earthquakes have been intensively studied from the world records, as was that of the Japanese earthquake of March 29, 1928, by Stechschulte. The reversal of the time-distance curve has thus been firmly established.

Certain characteristics identify a record at once as having been due to waves from a deep focus. The phases are more numerous and are sharply defined. There is a markedly smaller surface phase. The interval between the arrival of the direct longitudinal and transverse waves is abnormally long for short epicentral distances.

The characteristics of earthquakes from deep foci are no less pronounced than those of their seismograms. Although the latter prove the shock to have been very severe, the surface disturbance, even at the epicentre, is relatively small. Wider areas feel the tremors more than is the case for shallow-focus shocks. The direction of the motion at or near the epicentre is noticeably vertical.

The above paragraphs do not, by any means, complete the list of evidence proving the occurrence of foci at considerable depths. It is not the province of the present paper to detail these nor to outline the various means for determining the depth. The above sources of evidence have been given as a brief indication to those approaching the subject of this symposium from other fields of research that the fact of deep foci has been established beyond doubt and that the determinations of epicentres and focal depths for a large number of these have been carried out with at least fair accuracy. Some, studied in great detail, serve to check the rougher estimates made for others. A

great mass of data has been gathered in the two papers by Gutenberg and Richter entitled "Materials for the Study of Deep-focus Earthquakes". Similar lists were published earlier by Turner, Wadati, Berlage, Stoneley, Sharpe, and others.

We are here concerned with the geographical distribution of the epicentres of earthquakes and with their focal depths; for these are inescapably related on the one hand to the cause of earthquakes and, on the other, contribute to our knowledge of the interior of the earth. But before discussing this ultimate phase of the subject we must outline one further pertinent phase of seismological evidence.

In some earthquakes the direction of the initial movement of the ground at a distant station, due to the arrival of the first longitudinal wave, is down or toward the epicentre, in others it is up or away from the epicentre. Movements of the first type are called kataseismic, those of the second type anaseismic. It has been found that earthquakes show anaseismic motion at some stations and kataseismic motion at others. Further study has revealed other facts about this distribution which are exceedingly interesting but which do not contribute to the present subject. One feature does do so, however. It has been found that initial motions at any given station for all foci in a given epicentral region are almost always either kataseismic or anaseismic. The implication of this observation is far reaching.

It was early suggested that deep-focus earthquakes might be due to explosions at depths so great that the lack of strength in the rocks would, it was argued, preclude the possibility of slippage along fault planes. If that were the case, then all such deep-focus earthquakes should always record as anaseisms. As they do not, the motion being kataseismic at stations in some azimuths from the epicentre and anaseismic in the others, we are almost forced to conclude that the mechanism of plutonic earthquakes is the same as for those originating at lesser depths, i.e., slippage on a fault plane. The strong transverse waves generated from deep foci are further evidence supporting this assumption.

We come now to the distribution of earthquake epicentres and their foci. Before any distinction was made between the two classes of foci, many earthquake maps were constructed. These showed with increasing emphasis, as the more accurate seismological data

accumulated, that, in general, the regions of greatest seismic activity were closely associated with mountain building. A belt of high seismicity circles the Pacific. Another crosses through the Dutch East Indies across the north of India to the Mediterranean. A third, of minor importance, begins near Iceland and traverses the mid-Atlantic. Also, as time went on, a further truth emerged with considerable emphasis,—severe earthquakes sometimes occur quite outside these belts.

We now speak of earthquake foci at depths up to about 60 km. as "normal", those in the approximate range 60-250 km. as "intermediate", and those at 250 km. and greater as "deep". While normal and intermediate earthquakes may occur outside the seismic belts, deep foci occur only in those belts and only in five general regions thereof, namely: Japan, the Dutch East Indies, New Guinea, the New Hebrides, and in the western part of South America, respectively. We may examine in some detail the Japanese group.

In his earlier papers, Wadati concluded that it was possible to draw lines of equal focal depth through the epicentres of earthquakes plotted on a map of the Japanese islands. These lines, which might be called "isobathyseismals", were, as he drew them, wide arcs roughly parallel to the western edge of the Kurile-Japanese deep. Later, Wadati abandoned the isobathyseismals but defined two deep-focus zones. One of these lies approximately northwest-southeast or transverse to the main Japanese island, Honsyu, a little south of Tokyo, and extends up into the Sea of Japan and down into the Pacific. A second zone, of lesser importance, runs almost at right angles to the first from its upper end and bears roughly northeast across the northern tip of the second largest island, Hokkaido.

The plotting, by Gutenberg and Richter, of the locations, carefully computed from all the reliable data available, confirm these two zones defined by Wadati but reveal a striking fact: that the deep foci all lie on the edge of the zones farthest from the Pacific. This same arrangement is found for the Dutch East Indies region and for South America where the deep foci are found to the east of the Andes. The shallow foci lie nearest the ocean deeps and the intermediate ones between the other two. So far, the South American data reveal no foci between 290 and 600 km.

It must be noted that data on depth of focus are not available for more than thirty years past and that only during the last decade have they been of present-day accuracy. It is therefore not very safe to generalize. However, for what it is worth, we may sum up by saying: that the fact of deep foci is well established; that various criteria for determining depth are known and yield concordant values which are of high percentage accuracy; that the deep foci of which we so far have record are confined to the five regions mentioned; that they are associated with intermediate and shallow foci in a distribution which roughly shows the deep foci farthest from the oceanic deeps and the shallow foci nearest; that the deep foci show anaseismic and kataseismic properties which are generally the same as for intermediate and shallow foci in the same region; and that deep foci show strong transverse waves on their seismograms.

The interpretation placed on the above is that, in the five regions mentioned, there are deep, oblique, thrust faults which we might call geo-fault-planes and that all the foci result from slippage on these great planes.

Accepting this interpretation, we may examine a little more closely the phenomena of deep foci. It has been found that these "repeat" as do shallow foci. That is to say, shocks of almost equal intensity are recorded from the same focus at intervals of two years and less. If the interpretation of the paragraph next above be true, then forces must be at work building up strain at depth at a remarkably high rate. Moreover, the fact of repeat shocks is a mortal blow to the theory of an explosive cause for these deep foci.

It is also found that aftershocks occur in connection with deep foci as they do for those more shallow; again indicating a common mechanism. They are probably quite as numerous, but the smaller ones fail to record at the great distances due to the depth involved; just as small aftershocks at normal foci record only at near stations.

In certain regions the horizons at which the deep foci occur seem to be restricted. For example, in South America, many foci occur at depths from 600 to 660 km. with epicentres east of the Andes, while intermediate shocks at depths of 100 to 290 km. have epicentres in the Andean zone with strong normal shocks in the oceanic deeps and

the coastal plane. In other regions the distribution in depth is almost continuous, as in Japan and in the Dutch East Indies.

A strong deep focus earthquake occurs on the average about once a year. There seems to be no particular relation between their intensity and their depth, some of the strongest being at nearly 700 km. So far, again we repeat, so far, no depth appreciably greater than 700 km. has been found

This seems to present the case from the seismological viewpoint. The interpretation of these data comes within the scope of other papers of this symposium. It is perhaps not out of place, in conclusion, to point out that the laboratory experiments of Griggs, Bridgman, and others show that plastic flow of rocks at high confining pressures and temperatures exhibits a stop-and-go phenomenon which lends strength to the deduction that, even in the asthenosphere, the viscosity is so high that, if the forces build up with sufficient rapidity, slippage along fault planes could occur as in the lithosphere. The repeat foci show that the building up of these forces is remarkedly rapid. The question of the mechanism by which they so accumulate lies beyond the scope of the present discussion.

Dominion Observatory,
Ottawa, Canada,
September 9, 1941.

NORTH TEMPERATE BELT$_S$ MARKINGS ON JUPITER IN 1940-41

By Hugh M. Johnson

General Description.—It is well known that various latitudes over the observed surface of Jupiter maintain unequal rotation periods, somewhat less than 10 hours and having a range of several minutes, and that customarily the equatorial regions to latitudes about $\pm 10°$ exhibit the shorter periods. Accordingly, it has been found convenient to employ two rotational systems fixed by Marth in 1896,[1] System I for the equatorial regions and System II for the rest of the planet, their periods being:

I	9h.	50m.	30.003s.
II	9	55	40.632

But these adopted periods do not fit at times even approximately to the observed motions of markings in certain restricted latitudinal regions, the appearance of which interesting motions at present can be termed only "abnormal" for want of any knowledge as to their cause. One of these regions is at the south edge of the dark north temperate belt, latitude about $+25°$, known for its *very short* rotation periods near 9h. 49m., which have displayed at least five distinct manifestations since the first in 1880. Others followed in 1891, 1926 (one object), and 1929, as listed by B. M. Peek.[2] In September-October, 1939, Peek discovered a new series of small, dark projections from the south edge of the north temperate belt, which appeared similar to the spots in the 1929 "outbreak" and which were soon found to have the rapid rotational motion. He has given an account of them in the 1939-40 apparition (to February, 1940),[3] using visual observations by five B.A.A. workers. A mean rotation period of 9h. 48m. 56s. for the 11 best observed spots was found.

Early in the 1940-41 apparition it was evident that the N. Temp. B.$_s$ markings had continued into a second apparition (not

[1] *Monthly Notices of the R.A.S.*, v. 56, pp. 395 and 517, 1896.

[2] *The Observatory*, v. 59, p. 280, 1936, with bibliography.

[3] *Journal of the B.A.A.*, v. 50, p. 270, 1940.

a unique case) and in this paper we deal with the 1940-41 observations of them made by five American amateurs while in the process of a general programme on Jupiter. Chiefly we were interested in deriving rotation periods, on a basis of visual transit observations for the determination of longitudes of individual marks, in the manner so successfully employed by the Jupiter Section of the B.A.A. and well explained by Peek.[4] In short, this method involves merely a record of the time at which any point of interest on the planet is estimated to be at the central meridian; a series of which records when reduced to longitudes System I or II may be translated into the rotation period of the mark concerned. Our group of observers, each of whom with his principal telescope and location is listed in Table I, together with his number of transits for the whole planet and transits of dark N. Temp. $B._s$ marks.

TABLE I

Observer, Telescope and Location	*Transits for Whole Planet*	*N. Temp. $B._s$ Transits*
T. R. Cave, Jr., 6-in. refl., Long Beach, Cal.....	21	1
C. Cyrus, 10-in. refl., Lynchburg, Va.....	34	2
W. H. Haas, 6-in. refl., New Waterford, O.....	618	40
H. M. Johnson, 8-in. refl., Des Moines, Ia.....	1181	104
F. R. Vaughn, Jr., 8-in. refl., Des Moines, Ia.....	179	16
	2033	163

One other member of the group, D. P. Barcroft, with 6-in. refl. at Madera, Cal., obtained 37 transits, none of which pertained to N. Temp. $B._s$ marks, however. In the following sections we consider these collected observations in detail, U.T. dates and times being used.

Rotation Periods.—On the chart, Fig. 1, dates 1940-41 against longitudes System I,[5] are plotted the 163 transits of N. Temp. $B._s$ dark spots, with graphs joining those transits which seem likely to belong to one series on a spot. Only five transits were without one of the 16 graphs or "drifts" thereby found. In Table II data

[4] *Journal of the B.A.A.*, v. 47, p. 153, 1937.

[5] Because of the rapid rotation of these marks, System I is more convenient to employ than is System II, despite the latitude being outside the normal domain of the former.

for the rotation periods of the 16 marks are listed; an identifying number correlating each mark in the chart and table has been assigned.

TABLE II

No.	*Dates*	*Number of Transits*	*Change of Long. (I) in 30 days*	*Rotation Period*
1	July 4 - Feb. 10	15	−52°	9h. 49m. 20s.
2	July 2 - Feb. 28	14	−51	9 49 22
3	June 30 - Feb. 19	18	−54	9 49 17
4	July 23 - Sept. 29	8	−63	9 49 05
5	Sept. 6 - Feb. 19	9	−58	9 49 12
6	Nov. 30 - Feb. 19	3	−53	9 49 19
7	July 21 - Dec. 23	9	−54	9 49 17
8*a*	June 26 - Mar. 1	18	−46	9 49 29
9*b*	Oct. 27 - Nov. 17	2	−54	9 49 17
10	June 15 - Nov. 8	15	−55	9 49 16
11*c*	Aug. 18 - Sept. 3	2	−53	9 49 19
12*a*	Sept. 1 - Mar. 19	7	−53	9 49 19
13	Aug. 29 - Jan. 11	10	−62	9 49 07
14	Aug. 20 - Sept. 24	4	−79	9 48 44
15	Sept. 1 - Feb. 23	13	−59	9 49 11
16	Oct. 26 - Jan. 11	11	−61	9 49 08

Mean: 9h. 49m. 14s.

Notes.—*a* Final section of drift very uncertain as chosen.
b Observed by Johnson only.
c Observed by Haas only.

The mean rotation period comes out one-third minute greater than the result obtained by Peek for 1939-40: there can be almost no doubt that the increase is real. It is interesting to note that this deceleration apparently was most effective *between* the apparitions, *i.e.*, from January-February to June, 1940, for *in general* the graph forms on both Peek's and the present chart are linear. This deceleration in motion, probably irregular, together with a possible increase in the number of observed spots, would make practically impossible an attempt to identify any of the 1940-41 objects with those of 1939-40 by means of drift-extrapolation. In fact, we do not know whether any of the 1939-40 individual spots is a survivor over the three or four months when the planet was not observed, but surely we may safely call the 1940-41 group as a whole a continuation of the eruption of the previous apparition.

There are certain sinuosities in the drifts which probably represent actual variations in the rates of motion of the spots more than errors of observation, for it is believed that chance errors in visual transit work are of an order less than five minutes or three degrees longitude.[6] In spots Nos. 12, 13, 14, and 15, we seem to

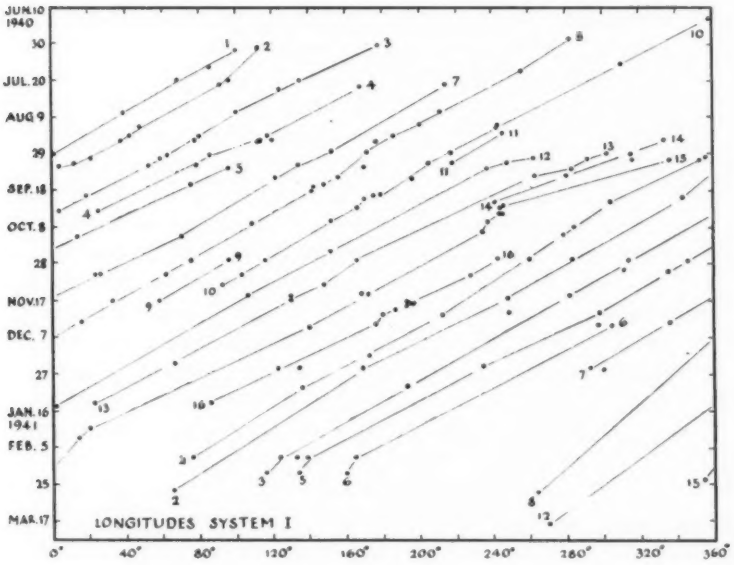

Fig. 1.—Longitude drifts of N. Temp. B_8 spots.

have cases of extraordinary motion, though it must be admitted that the interpretation of drifts among their early transit plots is tentative and requires confirmation. Briefly, however, these four spots appear to have originated between longitudes $262_1°$ and $336_1°$ at a later period (August 20 to September 1) than most of their fellows, and to have begun with "abnormally" rapid motion, decelerating to "normal" motion in a few weeks. There is good

[6]But all N. Temp. B_8 transits before August 15 (which are by Johnson) may be systematically early by about five minutes.

evidence that spot No. 15 was *abruptly* slowed between September 27 and October 10. Spots Nos. 14 and 15 were peculiar in being "quite (or) very small" at their first transits, but Nos. 12 and 13 were apparently of medium size.

Distribution.—Hardly more than a glance at the chart, Fig. 1, is enough to suggest that the number of N. Temp. $B._s$ spots may be distributed unevenly with respect to time, possibly with a periodic concentration. While it seems worth while to investigate the idea more carefully, it must be remembered that very precise information cannot be obtained because of the lack of sufficiently numerous and standardized observational data.

The simplest method for determining real variations in number of N. Temp. $B._s$ spots is a count of their number per unit of *observing* time. However, this requires an exact knowledge of the time spent observing each night for each observer, which we do not have, and it neglects spurious effects on the visibility of the marks resulting from variations in seeing, telescope, etc. But let us assume that the total real number of marks on the remainder of the planet remains constant, and that real numbers of N. Temp. $B._s$ spots and extra-N. Temp. $B._s$ spots are reduced to observed numbers by a visibility factor common to both groups, all approximately. Acceptance of these plausible assumptions enables us to express real variations in number of N. Temp. $B._s$ spots by the ratio $n/(N-n)$, where n and N are the *observed* numbers, per unit of time, of N. Temp. $B._s$ spots and all surface marks, respectively. Choosing the interval of time 10 days[7] in which to group transit numbers, and noting that the first N. Temp. $B._s$ spot transit came on June 15, 1940, and the last on March 18, 1941, we make 28 consecutive intervals the first of which begins June 15, 0h. In Table III we tabulate all the transits falling in these intervals; column one giving interval number; column two, limiting interval dates; column three, number of N. Temp. $B._s$ spot transits; column four, number of all transits; and column five, $n/(N-n)$.

[7]This interval should be large enough to include significant numbers of transits yet small compared with the frequency of variation we can investigate.

TABLE III

No.	*Dates, 0h*	*n*	*N*	$n/(N-n)$
1	June 15-25	1	13	.083
2	June 25—July 5	4	63	.068
3	July 5-15	3	107	.029
4	July 15-25	6	84	.077
5	July 25—Aug. 4	1	103	.010
6	Aug. 4-14	5	191	.027
7	Aug. 14-24	14	178	.085
8	Aug. 24—Sept. 3	16	168	.105
9	Sept. 3-13	17	149	.129
10	Sept. 13-23	8	113	.076
11	Sept. 23—Oct. 3	10	95	.118
12	Oct. 3-13	5	64	.085
13	Oct. 13-23	4	26	.182
14	Oct. 23—Nov. 2	10	73	.159
15	Nov. 2-12	8	76	.118
16	Nov. 12-22	12	84	.167
17	Nov. 22—Dec. 2	11	75	.172
18	Dec. 2-12	0	21	.000
19	Dec. 12-22	3	22	.158
20	Dec. 22—Jan. 1	5	56	.098
21	Jan. 1-11	2	46	.045
22	Jan. 11-21	3	43	.075
23	Jan. 21-31	1	17	.063
24	Jan. 31—Feb. 9	1	34	.030
25	Feb. 9-19	5	33	.179
26	Feb. 19—Mar. 1	5	37	.156
27	Mar. 1-10	1	18	.059
28	Mar. 10-20	1	20	.053

The tabulated ratios in the last column of Table III seem to show that quite significant variations in real number of N. Temp. $B._s$ spots occurred. These variations are more clearly shown in a graph of the form in Fig. 2, where abscissae are time and ordinates the ratios of column five, Table III. This graph may be interpreted as *roughly* representing[8] a complex curve whose principal characteristic is a short period frequency of very nearly 30 days. The evidence for reality of this period seems fairly strong. It will be noted that we supposed the phases of these variations occurred simultaneously

[8]Perhaps within the errors of the observations and methods by which it was derived.

in all longitudes. The more secular variations are certainly due in part to the observers' varying personal attention; *e.g.*, Haas recorded no N. Temp. $B._8$ spots until the seventh interval though he contributed other transits in the first six intervals. Table II

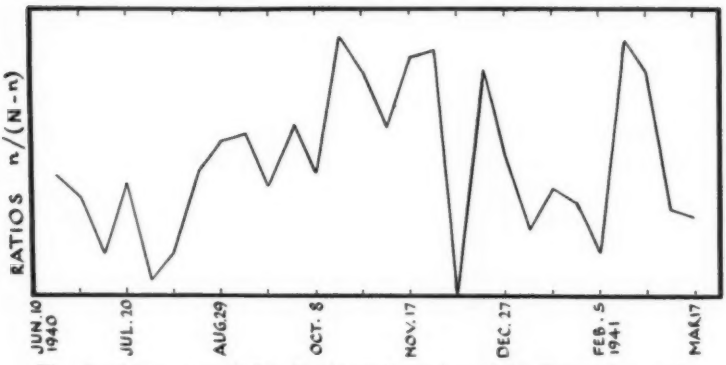

Fig. 2.—Apparent variation in observed number of N. Temp. $B._8$ spots.

also indicates that the real number of these marks simultaneously existing also varied, though we must not take limiting dates of observation for limiting dates of existence, necessarily.

1118 W. 26th Street, Des Moines, Iowa.
September 7, 1941.

A RECALIBRATION OF THE STANDARD LAMPS OF THE DOMINION ASTROPHYSICAL OBSERVATORY

By ANDREW MCKELLAR and R. M. PETRIE

THE Dominion Astrophysical Observatory has had in its possession for some time four standardized, ribbon-filament, tungsten lamps. These lamps were obtained from Dr. W. E. Forsythe of the General Electric Laboratories at Nela Park, Cleveland, and their colour temperatures for various operating currents were determined by him. Only the first of these lamps to be received has been used to any extent, it being employed by H. H. Plaskett in his study of the application of the wedge method to photographic spectrophotometry.[1] It is planned to use these lamps in connection with a number of studies involving heterochromatic photometry, which members of the staff plan soon to commence. Among these projects are the determinations of colour temperatures and spectral intensity distributions for early type stars. For a complete study of this sort, it is necessary to employ a source of known spectral emissivity, such as we have in a calibrated standard lamp.

Through the courtesy of Professor F. A. Jenkins of the Department of Physics, University of California, who made available a quartz standard lamp, manufactured and calibrated by P. J. Kipp and Zonen of Delft, Holland, the opportunity arose of checking the calibrations of our lamps. Although these had, as indicated, already been calibrated, it was considered worth while to compare them with the quartz lamp. It is with this comparison of three of our lamps (the fourth had previously been calibrated for one current only) with the Kipp and Zonen quartz lamp that this present short communication deals.

The comparison was carried out photographically, the single-prism spectrograph (dispersion $=52$ A/mm. at Hγ) ordinarily employed for calibrating stellar spectrograms being used. Using panchromatic emulsion, four plates were taken. On each plate was photographed the spectrum of the quartz lamp, hereafter

[1] *Pub. Dom. Ap. O.*, v. 2, p. 213, 1923.

referred to as lamp *a*, then in turn, our lamps *b*, *c*, and *d*. Also, each plate was calibrated photographically with the rapidly rotating sector usually used for calibrating stellar spectrograms. This calibration is in the form of a series of continuous spectra with a known relative intensity scale supplied by the relative sizes of the sector steps. On any one plate the exposure times for the four lamps were equal and all of the lamps were operated at about the same colour temperature. The four plates corresponded to lamp filament temperatures of approximately 1650° K, 2050° K, 2500° K and 2650° K. Electrical energy for the lamps was supplied by two 6-volt, 100-ampere-hour storage batteries and the current was adjusted with a suitable rheostat. The lamp current was measured with a Weston direct current ammeter.

The relative photographic densities of the spectra of the four lamps were measured at numerous wave-lengths with the observatory's microphotometer used in the direct reading form, and were reduced to relative intensities using the sector calibration. Such measurements were carried out for various wave-lengths between λ 3900 and λ 6400. On the plate corresponding to the greatest lamp current, measurements were made at thirteen wave-lengths between these limits, on the next two, at nine wave-lengths, while on that with the lowest lamp current, only six wave-lengths between λ 3900 and λ 4900 were utilized. This latter plate was somewhat over-exposed in the red region, because of the lower temperature of the lamps.

The method adopted for reducing the relative intensities for different wave-lengths, obtained as above, to relative colour temperatures was that used by the Greenwich observers[2] in their work on stellar colour temperatures. They showed that if, for black-body radiation, the logarithmic intensity-ratios, $\log_{10} I_n/I_{standard}$ were plotted against $1/\lambda$ (λ in microns) there results a straight line, the slope of which determines the colour temperature of the source giving rise to I_n, in terms of the colour temperature of the standard source. In the present case the quartz lamp *a* was taken as a standard for the determination of the temperatures of the other three lamps. Hence, plotting $\log_{10} I_n/I_a$ against $1/\lambda$, from

[2] *Observations of Colour Temperatures of Stars*, Royal Observatory, Greenwich, 1932.

the slope of the resulting straight line the temperature of the lamp in question was determined in terms of that of lamp a. The equations in the form used were,

$$\Phi_n = \Phi_a - 2.303 \frac{d}{d(1/\lambda)} \log_{10} (I_n/I_a)$$

where

$$\Phi_n = \frac{c_2}{T_n}\left(1 - e^{-\frac{c_2}{\lambda T_n}}\right)^{-1}, \quad \Phi_a = \frac{c_2}{T_a}\left(1 - e^{-\frac{c_2}{\lambda T_a}}\right)^{-1}$$

and $-2.303 \dfrac{d}{d(1/\lambda)} (\log_{10} I_n/I_a) = \Delta\Phi$ and is called the "relative

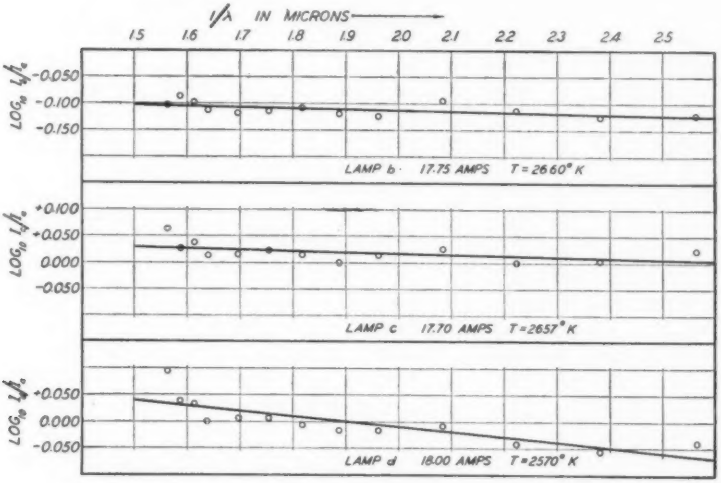

Fig. 1.—Relative Gradients of Lamps b, c and d with Respect to Lamp a Operated at 14.40 Amperes, T=2680° K.

gradient." In these expressions, the subscript a refers to lamp a, the subscript n to lamp b, c, or d, c_2 is the constant in Wien's displacement law, taken as 14,320 microns degrees, and T is the absolute temperature.

In carrying out the computations to obtain the relative gradients, least-squares methods were used. Graphical representations of the three gradients obtained from the plate corresponding to the highest lamp temperatures are shown in Figure 1. This figure

serves to give some idea of the correspondence between the slope of the straight line and the relative temperature while the sensitiveness of the method is indicated by the relatively great change in slope corresponding to a change of 90° in temperature.

The results from the four plates are summarized in the accompanying table and need little explanation. It should be pointed

INDIVIDUAL RESULTS OF LAMP CALIBRATIONS

	Lamp	Current in Amps.	Colour Temperature from Original Calibration	$\frac{d}{d(1/\lambda)} \log_{10} (I_n/I_a)$	Colour Temperature from Lamp *a*
Plate 1	*a*	8.00	1730° K	—	—
	b	9.40	1675	-0.169 ± 0.011	1655 (±5)°K
	c	9.90	1765	-0.164 ± 0.037	1655 (±16)
	d	10.20	1740	—	—
Plate 2	*a*	9.85	2083	—	—
	b	11.90	2000	-0.043 ± 0.028	2050 (±20)
	c	12.65	2120	$+0.032 \pm 0.017$	2105 (±12)
	d	13.20	2114	-0.025 ± 0.020	2065 (±14)
Plate 3	*a*	12.25	2420	—	—
	b	16.20	2460	$+0.108 \pm 0.012$	2525 (±12)
	c	16.00	2480	$+0.114 \pm 0.010$	2530 (±10)
	d	16.00	2410	-0.005 ± 0.008	2415 (±7)
Plate 4	*a*	14.40	2680	—	—
	b	17.75	2620	-0.018 ± 0.006	2660 (±7)
	c	17.70	2665	-0.023 ± 0.010	2655 (±11)
	d	18.00	2585	-0.100 ± 0.015	2570 (±16)

out, however, that the probable errors of the temperatures given in the last column (enclosed in parentheses) are those derived from the probable errors of the slopes listed in the previous column. Therefore, they are merely a measure of that part of the uncertainty in the temperature introduced by the scatter of the points defining the linear relation between $1/\lambda =$ and $\log_{10} I_n/I_a$ and thus take no account of the possible errors of measuring the current through the lamps. Taking 0.2 amps. as a liberal estimate of a probable

upper limit of the error in measuring the lamp current, the corresponding uncertainty in temperature would be about 20° K. Hence, the actual probable errors of the temperatures given in the

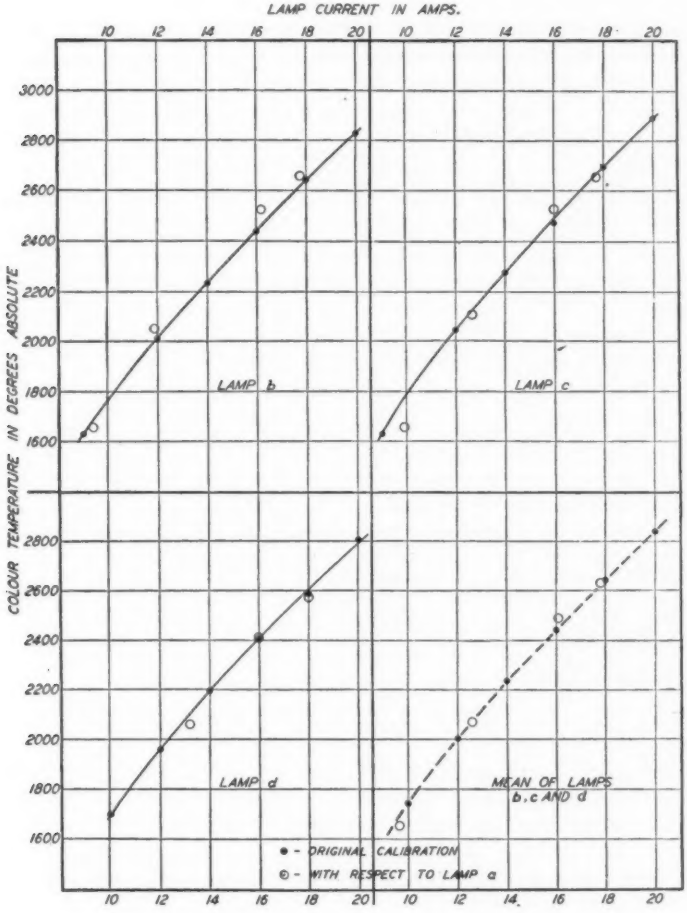
LAMP CURRENT IN AMPS.
COLOUR TEMPERATURE IN DEGREES ABSOLUTE
LAMP b
LAMP c
LAMP d
MEAN OF LAMPS b, c AND d
• - ORIGINAL CALIBRATION
© - WITH RESPECT TO LAMP a

Fig. 2.—Calibration Curves for the Standardized Lamps.

last column of the table should be about ±30° K. Had more plates been secured this figure, of course, could have been materially decreased.

The results are shown perhaps better in Figure 2, where the calibration curves of lamps *b*, *c*, and *d* are plotted. The points originally furnished by Dr. Forsythe are shown as small filled circles, while the temperatures obtained with respect to lamp *a* are represented by open circles. In the lower right-hand section of the figure is drawn the mean curve of the three lamps and on it the mean values of the colour temperatures are plotted. Obviously the scatter of the points determined in the present investigation is greater than that of those determined in the original calibration of the lamps. However, this does not seriously affect the purpose of the present work, which was to ascertain if any measurable systematic difference existed between the calibrations of our lamps, performed in Cleveland some fifteen years ago, and the calibration of the quartz lamp carried out recently in Holland. It appears safe to conclude from Figure 2, that, at least within the accuracy of the present measurements, no such systematic difference does exist.

The result of these calibrations is gratifying, since the agreement between the two sets of quite independent determinations is satisfactory. These lamps may now be used as a basis for a system of spectrophotometric studies with considerable confidence that any remaining uncertainty in their spectral distributions will not materially affect the results of such investigations.

Dominion Astrophysical Observatory
Victoria, B.C.
January, 1941.

CALENDAR REFORM

By C. E. White

THE reform brought about by the Julian and Gregorian Calendars was mainly for the purpose of measuring the length of the year more accurately and dividing this length into twelve months. Julius Caesar divided the year into months having respectively:

31, 29, 31, 30, 31, 30, 31, 30, 31, 30, 31, 30 days....(1)

for common years. For leap years he gave February thirty days. Augustus Caesar, in order to give the month August thirty-one days, changed the series in (1) into:

31, 28, 31, 30, 31, 30, 31, 31, 30, 31, 30, 31 ....(2)

He gave February twenty-nine days in leap years.

The Gregorian Calendar did not change the above arrangement into months, but dropped ten days from the Julian reckoning; and to prevent the accumulation of error caused by counting the year $365\frac{1}{4}$ days in length instead of 365 days, 5 hrs. 48 mins. 46 secs., it requires that century years must be a multiple of 400 to be a leap year.

Calendar reform, as proposed now, has for its purpose a change in the number of days in a month and a change in the position of the weeks so that there will be a close relation between the date and the day of the week. The main purpose of this is to make the determination of the calendar date of the current day a simple mental process.

Two methods are advocated for calendar reform: one, known as the Eastman[1] plan, would divide the year into thirteen months of four weeks each. It lets the extra day in a common year be a holiday which is not to be counted as part of a month or week and leap years are given two such holidays. The other, emphasized by the World Calendar Association, divides the year into twelve

[1]Because it was earnestly advocated by the great industrialist and philanthropist George Eastman. Before him Moses Cotsworth strongly urged it, but several others had proposed it still earlier.

months of thirty and thirty-one days as follows:

31, 30, 30, 31, 30, 30, 31, 30, 30, 31, 30, 30 ....(3)

with one or two isolated holidays as in the Eastman plan.

The Eastman Calendar would have each year and month begin with Sunday and the World Calendar would have each year and each quarter-year begin with Sunday, the second month of each quarter begins with Wednesday and the third month with Friday. From the view-points of mental calendar and accounting, the Eastman plan would be preferable, but when all things are taken into consideration the World Calendar is much to be preferred. Thirteen does not have aliquot parts while twelve has its half, its third, its fourth, and its sixth. In the World Calendar, the quarters are each thirteen weeks in length. A number of accountants and statisticians would be assisted by this simpler division of the year, but not enough to give this any weight as argument. Statistics enable the stock holders to know how the business is progressing, but extreme accuracy would not be essential for this purpose. If business for a thirty-one-day month is compared with business of a thirty-day month it is not in general necessary to mention the extra day. Business may adopt a calendar suited to its needs. For over one hundred years the banks have counted 360 days as the interest year.[2]

The changes made in historical dates and anniversaries by the World Calendar are practically negligible. In common years dates in the first two months and the last four months would remain unchanged, days in March and May would fall two days earlier. June, July and August would come one day earlier. Since there could not be an error of more than two days it would be wise in case of adoption to hold on to the original date. The date more than the anniversary is significant. I was born after midnight, October 25, of a leap year. Hence, the anniversary of my birth comes on October 24, three years out of four. But why be so accurate in such matters? October 25 is my birthday and there it stays. The Eastman plan would cause a very objectionable shake-up of dates.

The most serious objection to either plan would come from the fact that the Lord's Sabbath would be destroyed in the sense that

[2]The large merchandising firm of Sears, Roebuck & Co. did try the Eastman Calendar one or more years and found it objectionable.

many regard the sabbath. Time, from remote antiquity, from creation as many think, has been measured by an unbroken chain with each link a week of seven days. Many think that the seventh day was arbitrarily set aside as a fixed day for worship and rest. The Seventh Day Adventists, The Seventh Day Baptist and other Christian denominations and Jews so regard it and many of the laity in any church, especially the youth so regard it. Most Christian churches worship on Sunday, the first day of the week—a fixed day determined by the resurrection of Christ. The World Calendar breaks the chain each year with its isolated holidays and the Sabbaths, so far as the Hebrew week is concerned, do not remain fixed but may fall on any day of that week. "Remember the Sabbath Day, to keep it holy," was spoken of an absolute day. If this conception of the Sabbath be changed what will be the effect on Christianity, on Christian faith and on the Church, and would there not be an Ecclesiastical Calendar to preserve this conception of the Sabbath? We may believe that the Sabbath was made for man and that using any day of the week for Sabbath would meet with God's approval, but we should recognize that many, perhaps a majority, do not believe as we do. We also believe in freedom of worship, and should not approve of changes that will interfere with it. Because of the way the church is involved such changes in the calendar are not a question for scientists or a majority to decide—the laity in each church should have a say.

In the Eastman Calendar, Sundays would come on the 1st, 8th, 15th, 22nd and 29th of the month; Mondays on the 2nd, 9th, 16th, 23rd and 30th, etc. So the method of getting the day of the month from the day of the week would be very simple. In the World Calendar, dates for the first month of each quarter could be found as in the Eastman Calendar. For the second month of each quarter Wednesday would be the 1st, 8th, 15th, 22nd and 29th, and from these dates the date of any other day in the week can be found by adding or subtracting 1, 2 or 3. A similar method could be used for the other months. The letters *S*, *W*, *F*, might be called the key letters and the month numbers 0, 3, 5 as the key numbers. The key letters could be used as described above, counting Sunday the first day of the week, any date can be found by date $=w-m$, where w is the day number and m is one of the

numbers 0, 3, or 5. The above formula would hold for any year of any century.

To avoid objections from the church, a modified form of the World Calendar could be used. I suggest that the months be given respectively:

31, 30, 30, 31, 30, 30, 31, 30, 30, 31, 30, 31 days....(4)

for a common year and I would give December thirty-two days the year before leap year. This arrangement would make the month number of each quarter of a year the same. There would be 13 weeks in each quarter except the last, and this could for business purposes be made thirteen weeks by giving the employees one or two days of vacation.

Let us assume that the method had been put into effect in 1940 and instead of giving 1940, 366 days give 1939, 366 days. The purpose of doing this is to make a method I derived in the July-August 1941 number of this JOURNAL applicable to the new calendar; and the purpose of giving the year preceding leap year an extra day is to avoid the correction for January and February as in the present calendar for leap years, and also for the purpose of making the first three quarters of any year thirteen weeks in length as in the World Calendar and also to make the key numbers and key letters the same for each quarter.

The year 1940 began on Monday. We give the Monday to 1939 and start the new calendar with Tuesday. From the formula, $\text{date} = w - m$, where w is the day of the week[3] counting Monday 1, and m is the month number, there is no correction for 1940. Since Jan. 1 was on Tuesday, $1 = 2 - m$ or $m = 1$. Since Feb. 1 was on Friday, $1 = 5 - m$ or $m = 4$. Since March 1 was on Sunday, $1 = 7 - m$ or $m = 6$. Hence the key number of the year is 146 and the key letters T, F, S'—S to represent Saturday and S' to represent Sunday. Either can be used to find the calendar date.

It is Thur. in Jan., Apr., July, or Oct.; what is the date? Here $d = 4 - 1 + 7n = 3, 10, 17, 24, 31$.

It is Wed. of the second month of each quarter; what is the date? In this case $d = 3 - 4 + 7n = 6, 13, 20, 28$, and so on.

[3] In the article "Perpetual and Mental Calendars," it is shown that the day number is easily found from the abbreviations: Mon. 1; Tu. 2; Wed. 3; Thur. 4; Fri. 5; Sat. 6; Sun. 7.

For the year 1941, we must add the correction $2q-r$[4] where q and r are the quotient and remainder found by dividing $y-40$ by 4; hence for 1941 $2q-r=-1$, the correction, or 1 must be added to each of the digits in 146 to get the key number for 1941. Hence 250 is the key number for 1941. It would probably be better to let the correction be the number to be added to each digit or $r-2q$. The correction for 1942 is 2 and the key number becomes 361. To use this method it would be necessary to remember the initial key number and add to its digits the correction for that year. Observing this, the new calendar could begin any year. No matter how simple the calendar there will be wall calendars and from these calendars it would be easy to find the key number for that year by the formula $m=w-d$. If w be the day of the week of New Year's day, $m_1=w-1$, $m_2=w-1$ where w is the day of the week of Feb. 1, and $m_3=w-1$ where w is the day of the week of March 1, or for any year $m_2=m_1+3$ and $m_3=m_1+5$. Observe that the key number consists of month numbers plus their corrections.

However, for the proposed calendar, it is convenient to speak of these as month numbers and the key number as the month key number because instead we can use a day key number to get the date. Thus, since for Jan. 1, $1=w_1-m_1$ or $w_1=m_1+1$. In like manner $w_2=m_2+1$ and $w_3=m_3+1$. Thus for this year 1941, the

[4]In the article, "Perpetual and Mental Calendars," I emphasized this as a correction for any year following 1940 of this century, but I failed to observe that it would be the correction for years previous to 1940 of this century if we observe that q and r are negative, and that 1 must be added for common years. Observing the series from which the method was derived, when taken in reverse order 2 instead of 1 must be added in passing from a leap year to a common year. The formula date

$$=w-m+2q-r-c+7n.$$

Where $c=0$ for this century and $c=6-2\left[\frac{C}{4}\right]_r$ for any century or where $-c$ takes consecutively values, 0, 1, 3, 5, 0, 1, 3, 5 . . . and where 1 must be added for common years when $y<40$ will hold for the proposed and present calendar except for the present calendar 1 must be added for Jan. and Feb. of leap years when $y>40$ and subtracted, when $y<40$. In the proposed calendar it is more convenient to regard $2q-r-c$ as the correction because the value of this correction is as easily found from the wall calendar or the known week day of any date as it is to find $2q-r$ for this century.

month key number is 250 and the day key number is 361. The day key number tells us that the 1st, 8th, 15th, 22nd and 29th fall on Wed. in Jan., Apr., July, Oct. Hence, to find the date of the current day add $w-3$ to one of these dates. The day key number shows the key letters to be W, S, M. We let S stand for Sat. and S' for Sun., T for Tu. and T' for Th. When we use the key letters no formula need be used. If we wish to find the dates of Sat. in Jan., Apr., July, and Oct. we think and count as we think, Thurs., Fri., Sat., and add 3 to one of the above dates or add 3 to 1 and then add a multiple of 7. With the exception that new key numbers must be determined for each year, the methods of finding the calendar date mentally are the same for the modified World Calendar as for the World Calendar.

The new key numbers may be determined from the preceding key number by adding 1 to each digit of the key number except that in passing from a common to a leap year 2 must be added. The following key numbers for 28 years beginning with 1940 have been so determined:

146, 250, 361, 402, 624, 035, 146, 250, 402, 513,
624, 035, 250, 361, 402, 513, 035, 146, 250, 361,
513, 624, 035, 146, 361, 402, 513, 624, 146.

I have taken 28, the Solar Cycle, because the numbers in the cycle will be repeated in the same order for each successive 28 years. With the exception of three numbers, each 513, each number either contains a 0 or 6—twelve zeros and thirteen sixes. This shows that there are four months of twelve years that begin on Monday and four months of thirteen years that begin on Sunday and four months of three years begin on Saturday. Hence, the dates of months that have 0 as month number can be found by $w+7n$, where w is the day number counting the week beginning with Monday; and the dates of months having 6 as month number can be found by $w+7n$, where Sunday is the first day of the week. We have observed that in the World Calendar for four months out of each year, $w+7n$ will give the date. So with respect to this particular feature, it is only the difference of three years out of 28 that might cause one to prefer the World Calendar. However, this is more than balanced by the fact that it is easier to determine

the day number when the week begins with Monday. Streamlining with Sundays is nice but there is not as much practical value in it as some think. In one way of looking at it, it would be nice to have all holidays come on Monday, but when one thinks of the increase in automobile accidents it is not so nice.

If we had to buy calendars and almanacs[5] or if we had to pay a high tax on them[6] there would be an advantage in a World Calendar because one would last a life-time and longer. But the calendar and almanac have for centuries been a medium for advertising. So without cost we get a wall calendar each year with a nice picture thrown in. Wall calendars could contribute to make the modified World Calendar popular by giving on the first page or on each page the month key number and the day key number. The key numbers should be memorized so that it will not be necessary to determine them but once. If w be the day of the week of New Year's day, $w+1, w+2, w+4$ will be the digits of the month key number and w, $w+3$, $w+5$ will be the digits of the day key number.

We can also get thirteen-week quarters by giving the months the following lengths:

30, 31, 30, 30, 31, 30, 30, 31, 30, 30, 31, 31 ....(6)

For leap years give January thirty-one days or give December of the preceding year thirty-two days. If thirty-one days were given January and the correction is found by formula, 1 must be added for January of leap years if $y>40$ and subtracted if $y<40$. If the key numbers are found independent of formula, January should not be used in finding them. For 1940 the key number would be 136[7] except that for January, 1 must be added to the date; or 036 should be used as the key number for the first quarter and 136 for the other quarters. For the present calendar, the formula

$$d=w-m+(2q-r-c)+7n$$

[5]The Chinese Imperial Almanac is the oldest. From time immemorial until recently the Chinese imperial astronomers have made yearly an almanac at the head of which is the emperor's edict by which all were forbidden under pain of death to use or publish any other calendar. Millions were sold annually.

[6]For a period during the eighteenth century taxes were so high in England on almanacs that publishers put much material in them so they could be sold as books.

[7]The key numbers for 28 years have 24 zeros and sixes.

can be used where m is determined from the series

$$144,\ 025,\ 036,\ 146 = 12^2,\ 05^2,\ 06^2,\ 12^2 + 2.$$

This series is easily remembered and visualized so you can easily find the right number to be used as m for the r month. In the article referred to above I showed how to find $(2q - r - c)$ by formulas, but it is easily determined from a wall calendar or a date on a newspaper by

$$d = w - m + \text{correction}.$$

I hold in my hand a newspaper dated Thursday, Sept. 11, hence $11 = 4 - 6 + \text{Cor.}$ Hence, $\text{Cor.} = 13 \equiv 6 \equiv -1$. Jan. 1 was on Wednesday, hence $1 = 3 - 1 + \text{Cor.}$ or $\text{Cor.} = -1$. The series is not easily forgotten if one uses it a few times. So, the only thing one needs to learn each year is one number, the correction. When this is taken into consideration the mental calendar for the World Calendar or for the modified World Calendar is not much more simple than the mental calendar for our present calendar.

Calendar reform also calls for a fixed date for Easter and I doubt if there would be opposition to a proposal of the kind. It appears it should be celebrated as an anniversary of the Resurrection. In an article on "Perpetual Ecclesiastical Calendars,"[8] I made a calculation to show that the Resurrection was March 27. Had the Julian Calendar been corrected by the Gregorian back to 1 A.D. or 31 A.D. instead of to 325 A.D., the Gregorian anniversary of the date would be April 9, but April 6 on correcting to 325 A.D. The Julian anniversary would be March 24 (April 6 − 13 days). If Easter should fall in March it would come at a time most suitable for a Spring vacation for students because it almost divides the time from Jan. 1 to June 10 into equal periods. Economically it would prevent much unwise expenditure of money—that rush and rash buying, which an April Easter causes. It would probably improve the quality of Spring fabrics.

It should be observed that all countries except Roman Catholic countries were very slow in dropping the Julian and adopting the Gregorian. Russia not till 1918, Roumania 1919, and Greece and Turkey still later. Practically all the civilized countries of the world and also a few uncivilized nations are now using the same

[8] In the July-August number of this JOURNAL, 1941.

calendar. This is fortunate, and any attempt to change may do more harm than good. The fact that the League of Nations is practically dead and the fact that it is hard to get agreements where there is so much hatred and hard to get a just hearing and action where dictators rule, an attempt to get a reform would not be wise for a number of years to come.

Rev. E. S. Schwegler, D.D., discussing reform of the calendar, says: "Calendar reform is being agitated by very powerful agencies. If it can be adopted by the church without essential difficulty, with many advantages both for herself and the World in general, members of the church should be the first to advocate it. If it offers insuperable obstacles, or should be combatted for any good reason whatsoever, then the opposition should begin now. It will be too late to offer objections if we ever come to the point where the state imposes a calendar upon everybody."

I am opposed to the adoption of the World Calendar except in a modified form that will prevent the continuity of Sundays being broken. I take this stand not because of any religious prejudice or views of my own but in defense of the rights and convictions of others. What reasons could the Jews and certain Christian denominations give for using Saturday as the day of worship instead of Sunday, and what reasons could any denomination give for worshipping on Sunday instead of Saturday or some other day of the week? In the first year after adoption Sundays will come as usual, but in the second year church members will get their eyes open to the fact that the chain of weeks has been broken and that the new links do not belong to the chain that, as many think, had its first link of seven days made by God. They will observe that they are worshipping on Monday of the old chain that was handed down by our ancestry and which we each inherited. What will happen if we destroy that heritage? Evangelists will proclaim that man has destroyed the Lord's day, that the Amageddon is here and the last days are not far off. The fourth-year Methodist will worship on Wednesday, Seventh-Day Adventists on Tuesday and during these two days business will get a set-back and even the wheels of factories may stop. It seems like nonsense to me that we should have some Protestants worshipping on Saturdays and others on Sundays, but what is nonsense

for one may be food for the soul of another and I am willing it should be. Since Saturday is a day off for most people, Saturday as a day of worship does not interfere so much with business. I suggest that a modified form of the World Calendar be tried first.

Bloomington, Indiana.

ASTRONOMICAL GRAPH SHEETS

By J. W. Campbell

FIGURE 1 shows the fundamental circles of the celestial sphere for an observer at latitude 53.5 deg. north. The use of such a figure in making a graphical transformation of coordinates is well known. However, there is a technique in using it which has not been pointed out in text-books so far as I am aware.

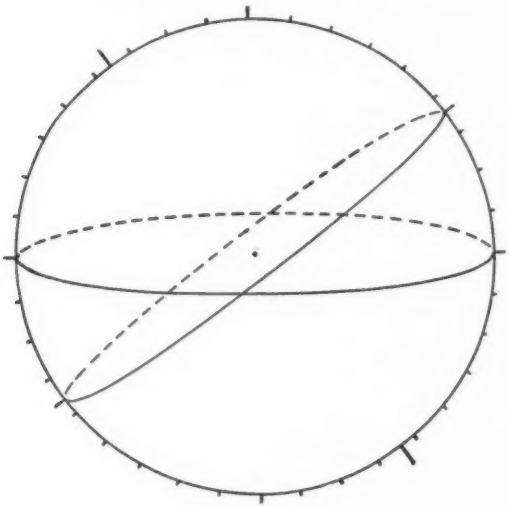

Figure 1

The three great circles shown all appear in perspective as ellipses, but the meridian circle is so nearly circular in the figure that arc measurements may be indicated by a uniformly graduated scale as shown. Then this graduation can be used to read off arcs on other circles which are foreshortened by perspective.

Thus, suppose A is the position of an object on the sphere (Figure 2), and that we wish to read off its hour angle and declina-

tion. It is easy to draw reasonably accurately the hour circle PAB through A. Through B draw the chord BC which is parallel to the diameter WOE. Then through the centre of BC draw the chord DD' parallel to the diameter POP'.

The points B, C, D, D' then all lie on a small circle of the sphere, and Q is the centre of the resulting segment. Hence the arcs QB, QC, QD, and QD' are all equal. The arc QB is therefore obtained by reading the arc QD and its value is 40.0 deg. The hour angle of A therefore is 2h. 40m. W.

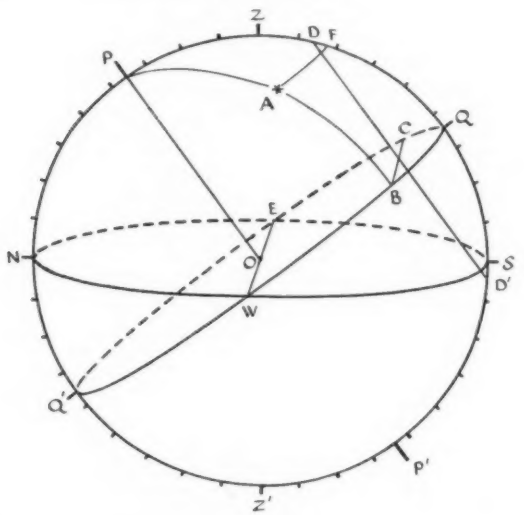

FIGURE 2

Again, through A draw an arc AF parallel to the equator WQ. Then the arc BA is equal to the arc $QF=30.6$ deg.; that is, the declination of A is 30.6 deg.

The reverse problem of locating the object on the sphere, when the hour angle and declination are given, is easily done by these same principles of construction. The same is true of positions with respect to the horizon system.

There is one point worth noting, however. The accuracy of

the arcs has to be judged by a visual estimate, as has also the parallelism of the chord BC and the diameter WOE, but the parallelism of the chord DD' with the diameter PP' can be checked by the fact that $QD = QD'$.

But to obtain good results in coordinate problems by geometrical construction it is essential, of course, that the fundamental framework be reasonably accurately drawn. If this has to be done by a student for each problem attempted, then much time and effort will be spent in this routine before the essential part of the problem is encountered. This difficulty can be obviated by the supplying of the fundamental framework in the form of graph sheets. Figure 1 illustrates the graph sheets which have been used at the University of Alberta for a number of years.

Such sheets can be modified to suit the latitude of the observer. Figure 1 is designed for the latitude of Edmonton.

NOTES AND QUERIES

Communications are invited, especially from amateurs. The Editor will try to secure answers to queries.

J. Edward Maybee (1866-1941)

In the death in Toronto on August 1 last of J. Edward Maybee, the Royal Astronomical Society of Canada lost from its ranks an amateur astronomer of merit. He joined the Society in 1896 and, being possessed of a telescope, took keen interest in observing which he continued until a few years ago. From time to time he presented a number of papers to the Toronto meetings and took part in its activities in the spare time of a busy life. He was one of the members of the Society chosen for the Government expedition to Labrador to view the total eclipse of the sun in 1905, and until recently he was the treasurer of the Toronto Centre of the Society. Born near London, England, in 1866, he came to Canada when a young boy and was educated in Toronto at Ryerson School and Jarvis Collegiate Institute. In 1893 he became a member of the firm of Ridout & Maybee, Patent Attorneys in Toronto, and on the death of his partner in 1910, became the sole proprietor of the firm and he so remained until about a year ago when ill health compelled his retirement.

Though his formal technical education was limited he was always a keen student and took more than a passing interest in all the physical sciences as well as astronomy, with the result that he was credited with having a comprehensive knowledge of mechanics, chemistry and electricity in particular. Yet his interests were not solely scientific and professional but embraced literature, history, art and music as well. To those that knew him and were best able to judge, he was regarded as an unusually well-informed man. He had the faculty, however, of concentrating on essentials and correlating his facts so that he never kept in his mind a mere collection of useless information. He also took a prominent part in other activities than those enumerated, in his younger days being an enthusiastic yachtsman and lawn bowler.

He was one of the founders of the Horticultural Society of Port Credit, a suburb of Toronto, where he lived for many years, and was interested in the Y.M.C.A. and Boy Scout movement there. He also served as chairman of the Board of Education for a number of years

and actively engaged in community service. The general regard in which Mr. Maybee was held by members of his profession was shown by the fact that he was elected as first president of the Patent Institute of Canada.

Just a year ago the "13 Club", the oldest literary and debating club of the city, tendered Mr. Maybee a complimentary dinner and presented him with an illuminated address in recognition of attaining his fiftieth year as an active member of the club.

The highest compliment that can be paid him after having enjoyed a well rounded life is to say truly that he was regarded supremely as a "man of honour."

Those surviving are: his widow, formerly Annie Augusta Short; a daughter, Lynette (Mrs. D. W. Ferrier); a son, W. H., now an instructor in No. 10 Elementary Flying School, Mount Hope, Ont., near Hamilton; and Gareth E., who succeeds his father in his profession.

J. R. C.

A Zeno Paradox in Himalayan Mountain Climbing

The paradoxes of Zeno (about 450 B.C.) are familiar to students of mathematics and philosophy. Perhaps the best known is that of Achilles and the tortoise. If the tortoise has a start of Achilles, then, argues Zeno, Achilles can never come up with the tortoise; for while Achilles traverses the distance from his starting-point to the starting-point of the tortoise, the tortoise advances a certain distance, and while Achilles traverses this distance, the tortoise makes a further advance, and so on to infinity. Consequently Achilles may run on to infinity without overtaking the tortoise.

Marco Pallis, a well-known English mountain climber, in "Peaks and Lamas" (1939) says:

> As recently as 1933 it was still generally believed that Europeans were only just capable of becoming sufficiently acclimatized to climb their peaks, and that, in the absence of native porters to do their fetching and carrying for them, they had hardly a hope of success. For one European climber to go high, several natives had to accompany him. It is not difficult to calculate what a multiplication of personnel and baggage resulted from this hypothesis: for the attendant natives themselves food and shelter, which entailed again more porters, and so on forever. In fact, it could be proved mathematically that by this pro-

cess one European party would eventually involve the whole human race, and still the problem would be no nearer solution.

The kindly southern Irishman is often credited with slightly twisting the truth in order to give an answer which he thinks will be pleasing to an inquirer. He will probably insist that the distance to Kilkenny is but five short miles when actually it is three times that. From the book just referred to it would seem that some Indians have the same would-be obliging nature:

> The Tibetans and allied peoples tend to be extremely accurate, while the Indians are inclined to romance, more from a desire to please than from any wish to deceive. There is much truth in the tale of the tired English traveller who asked the distance to the next village. "Not far", was the answer. "What do you mean by that? Is it about three miles?" "Yes, your honour." But the Englishman still entertained some doubts. "Are you sure it's only three miles? Or is it six?" "Yes, six, your honour." Losing his temper the Englishman cried: "What the devil do you mean by saying it's three miles and then six? Don't tell me any more lies, which is it, three or six?" "It's as your honour pleases."

Great Auroral Display

On the night of September 18-19 a very fine auroral display was observed at the David Dunlap Observatory. It covered practically the entire sky, and there were many darting streamers and waving curtains. The latter seemed most prominent in the north and east and at times their lower edges were tinged with pink. The position of the focus of the streamers was located with considerable definiteness. The display was first noticed by the writer about 9.30 p.m. and its rapid changes were followed for a considerable time. Later on, about 4 a.m., the motion of the sheets and streamers was still vigorous.

Death of John S. Plaskett

Dr. John S. Plaskett, who retired from being director of the Dominion Astrophysical Observatory in 1935, died on October 17. An account of his life and work will be given later.

C.A.C.

NOMINATIONS FOR THE CHANT MEDAL

IN 1940 the Royal Astronomical Society of Canada founded the Chant Medal, "in recognition of the long and outstanding services of Professor C. A. Chant to the Royal Astronomical Society of Canada." This medal is to be awarded "not oftener than once a year, to an amateur astronomer resident in Canada; the award to be made on the basis of the value of the work which he has carried out in astronomy and closely allied fields of original investigation."

The first award was made at the annual At-Home of the Society, in January, 1941, to Mr. Bertram J. Topham, of Toronto. Mr. Topham carried out research programs on variable stars, the aurora borealis, meteors and nova and comet search. He has also been interested in instrument construction and in popularizing astronomy through his private observatory.

The Council of the Society now wishes to call for nominations for the second award of this medal. Nominations for this medal may be signed either by the nominee himself or by friends of the nominee who feel that his work is worthy of such an honour. All nominations and supporting papers should be in the hands of the General Secretary, 198 College St., Toronto, by November 30. Nomination blanks (similar to that shown on the following page) may be obtained from the secretaries of the various centres or they may be secured from Society Headquarters at 198 College St.

It should be pointed out that the fundamental purpose of this medal is to stimulate and promote worth while work by Canadian amateurs in the field of astronomy, but work in allied fields of science (physics, chemistry, or meteorology for example) will not be excluded in a consideration of a candidate's qualifications for the medal. However, it is the value of his work in astronomy (considered in its broadest sense) which will chiefly determine his eligibility. It should also be understood that the medal is not awarded on the basis of work done for, or to advance the interests of, the Royal Astronomical Society of Canada, valuable as this work is. The Chant Medal is designed to encourage contributions to science itself rather than to any scientific society.

E. J. A. KENNEDY, *General Secretary* · FRANK S. HOGG, *President*

NOMINATION FORM FOR CHANT MEDAL

The Chant Medal is awarded by the Royal Astronomical Society of Canda not oftener than once a year to an amateur astronomer, resident in Canada. The award is made on the basis of the value of the work which this amateur has carried out in astronomy, and closely allied fields of original investigation.

Nominee .....
(Surname and given names in full)

Address .....

.....

If member of the Society, Centre with which affiliated .....

Age last birthday ..... Citizenship .....

Education:—(state the schools, colleges, or universities nominee has attended and list the degree and diplomas obtained)

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Occupation—(state general nature of present employment) .....

.....

Scientific Experience—(give details of any scientific positions or affiliations held in the past).

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Statement of Scientific Work Accomplished by the Nominee—(This statement should give a full account of the material which is to serve as a basis for making the award. It is understood that the nominee is still actively engaged in his astronomical work although the following account need not be restricted to what has been done during the past year).

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Signed:—.....

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Date.....

(This nomination should be signed either by the nominee or by those proposing his name, and must be mailed to the general secretary, Royal Astronomical Society of Canada, 198 College St., together with any supporting letters, so as to arrive before Nov. 30.)

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1890-1941

The Society was incorporated in 1890 under the name of The Astronomical and Physical Society of Toronto, and assumed its present name in 1903.

For many years the Toronto organization existed alone, but now the Society is national in extent, having active Centres in Montreal, P.Q.; Ottawa, Toronto, Hamilton and London, Ontario.; Winnipeg, Man.; Edmonton, Alta.; Vancouver and Victoria, B.C. As well as about 700 members of these Canadian Centres, there are over 200 members not attached to any Centre, mostly resident in other nations, while some 300 additional institutions or persons are on the regular mailing list for our publications.

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